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The Philosophical Foundations of Systematics

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At the time when the Entomological Society of Canada was founded, 88 years ago, the science of entomology was almost synonymous with systematics. I say almost synonymous, because important advances in the study of insect development and behaviour had already occurred and even economic entomology, which now absorbs nine-tenths of our professional effort, was already showing signs of growth. Nevertheless, the great names in entomology were still those of the systematists. The work of description and classification was still the most absorbing interest in the entomological field.

Systematics and the systematists are still with us as they must always be, if only because the exact identification of the materials with which we are working is of absolutely fundamental importance. But the increase in the number of systematic workers has not kept pace with the growth of the entomological corporation as a whole. Systematic staffs the world over, are woefully inadequate for the work they have to do. This is certainly not because all the insects have been described and classified. It may be partly due to the fact that the authorities concerned with entomological development do not open up enough positions in systematics. But a very important factor is certainly a general decline of interest in this branch of entomology. From many quarters we hear complaints that very few young people now wish to take up work in the systematic field. They have acquired, sometimes from their own teachers, the impression that though a lot of detailed work remains to be done, taxonomy as a creative effort, is played out; because its fundamental problems have long ago been solved. They therefore turn and are encouraged to turn, to newer fields like ecology and physiology which seem to promise richer rewards, both intellectually and materially.

I think this is a mistake. I feel that systematic biology is due for a revival and a renovation, not indeed with respect to the systems of classification and the working principles of taxonomy which I think are essentially sound; but rather in respect to the baggage of philosophical and theoretical ideas that systematists have acquired from various sources and are carrying around, (if I may mix my metaphors) as a chrysopid larva carries its cast skins. That there is something wrong somewhere is shown, I think, by the contrast one notices between the firm and sure moves of the systematists when they are engaged in their specific work and the vacillating and uncertain account they give of the object and methodology of taxonomy. I venture to think that the philosophical principles the systematists profess do not always agree very well with the philosophical principles by which they work and it seems to me that if the philosophical fog could be cleared away, new prospects, of considerable interest, would open out. In other words, I think that contemporary philosophy, or, at least, a misunderstanding of contemporary philosophy, has sometimes caused taxonomists to define their problems in a manner that is fruitless and futile. We must know how to frame sensible questions if we want to get useful answers.

My object in this paper therefore is to set out and justify, against some common misconceptions, the working principles of systematics and to show that, far from being a dusty and defunct science, it is full of interest for both the

scientist and the philosopher. This is a large order since a subject so vast and complex cannot be adequately covered in the time available but I hope at least to touch on some salient points. I am not at all sure that even the taxonomists among us will agree with me. If they do agree they may consider that I am simply retailing a collection of platitudes. However, I shall be satisfied if I can throw a little new light on a few of these old problems.

Taxonomy, as its name implies, is a kind of arrangement or ordering. The way in which our taxonomic activity starts has been vividly described by Cardinal Newman in his essay on the "Idea of a University". The exterior world, he suggests, first appears to us as a chaos, which we gradually resolve. "By degrees", he says, "by the sense of touch, by reaching out the hands, by walking into this maze of colours, by turning round in it, by accepting the principle of experience, the first information of the sight is corrected, and what was an unintelligible wilderness becomes a landscape or a scene, and is understood to consist of space, and of bodies variously located in space, with such consequences as thence necessarily follow". The same idea is expressed by Aristotle in the first book of the "Physics"[1] but Aristotle takes us a little deeper into the subject. "What is to us plain and obvious at first" he says "is rather confused masses, the elements and principles of which become known to us later by analysis". But he also says that when we follow this method, we are starting from the things which are "more knowable and obvious to us" and proceeding "towards those which are clearer and more knowable by nature". Thus, although our primary sensory apprehension reveals the world to us as a confusion, which the intelligence cannot assimilate as such, analysis reveals that it is composed of intelligible elements; and we can then see that what seemed to be a confusion is a system of relations involving these elements. Thus the knowledge of things in their causes, which is, for Aristotle, the goal of Science, reveals the world to us as a cosmos or order and therefore a suitable field for the taxonomist. These views manifest the philosophical optimism of Aristotle. This optimism is not blind or unreflective. The world is, on the whole, comprehensible, but not wholly comprehensible; the things of nature usually reach the ends to which they move; but sometimes fail to attain them. These reservations will have to be recalled when we consider the taxonomic system in detail.

The analytical process I have described is the common sense basis of systematics. Classification in this broad or general sense, is of immemorial antiquity. An orderly or systematic contact with material things is a practical necessity for human existence; because, among the diversity of objects about us some are useful to us, some are useless and some are positively harmful. The primitive hunter, the builder, the agriculturist, in connection with their practical operations, are constantly constructing classifications that are, indeed, crude and elementary, but nevertheless, greatly simplify their tasks. But classification is also an intellectual necessity. To know is one of the fundamental human appetites and classification is the method whereby the apparent flux and chaos of the external world are reduced to quiescence and unity and become the object of intellectual contemplation.

How far back we must go to find the origins of definite scientific classification, I have not attempted to discover. However, the outlines of a taxonomic system with a great deal of information about many living forms, clearly appear in the works of Aristotle and more particularly in the "Historia Animalium". But Aristotle had no immediate successors. It must be remembered that the ancient world was built on the institution of slavery and that the activities which

[1] 184a, 10-20.

bring us most closely into touch with nature were those which Aristotle himself classes among the "servile arts". During the long transformation of Europe after the decline of the Roman Empire, the slave gradually became a serf; but some free men also sank into serfdom. In a time when kings and great feudal lords could not even sign their names, and literature and learning were fostered almost only by the Church, the direct contact with nature, by those most fitted to collect and systematize the knowledge of living things, was largely interrupted. We must recall also that even Aristotle found it necessary to defend the study of the humbler animals against those who despised it as unworthy of a philosopher.[2]

However, as schools and universities developed and education became more general, treatises not only reproducing Aristotle and Pliny, but containing new, first-hand observations, began to appear. While the 13th century works of Albert the Great, Thomas of Cantimpré and Vincent of Beauvais, contain relatively little that is new, the 16th century produced some illustrated botanical treatises based on direct observation. The medical importance of plants, among other things, stimulated the advance of systematic botany as compared to zoology. In the 17th century, development was rapid. A work by Gaspard Bauhain, called the "Pinax Theatri Botanici" described 6000 kinds of plants.

But the appearance of a definite taxonomic science required not only an adequate collection of material but also an understanding of the principles involved in classification and the development of a method of applying the principles to the material.

The attempt to understand what is involved in the taxonomic process—I mean, the general process of sorting things out into groups containing things of the same kind and of establishing a hierarchy between such groups,—has a long history. This problem came into the Western world in the translation by the 5th century author Boethius (A.D. 480-524) of Porphyry's "Introduction" to a work called "The Categories", ascribed to Aristotle. King Alfred translated one of the works of Boethius[3] into Anglo-Saxon. His influence on western culture was great and it is due to him that Western philosophers began to work on the problems of material and formal logic before the natural philosophy and metaphysics of Aristotle became known to them through the Arabian commentators.

Through the work of Porphyry, the Western world was introduced, in the early stages of its intellectual development, to two matters fundamental to taxonomy: the fundamental taxonomic framework, supplied by the "categories": the fundamental taxonomic problem which is that of the significance of these categories and is usually called, the problem of the "universals". Perhaps it is not too much to say, that the great debate of the Middle Ages mainly concerned this problem of the universals.

The key-word with respect to the systematic framework is the word "species", which is the Latin translation of the Greek word "eidos". Now "species" is, for the logician, or, at least, for Porphyry, one of the five so-called "predicables", which are the ways in which the predicate of a proposition can be related to its subject; the other predicables being: genus, differentia, proprium and accident. The terms genus, species and difference are of special interest to us. *Genus* refers to the *class* in which the subject is *included*; the subject is a *species* belonging to this class; the characteristics which distinguish this species from other members of the genus, constitute its *difference*. We would say, nowadays, that they constitute its *specific characters*.

[2]De Partibus Animalium, Bk. 1, Ch. 5, 644b, 645a.
[3]De Consolatione Philosophiae.

Here we have a taxonomic framework which has a high degree of generality but is excessively simple. We can apply it equally well to books and beetles. It is too simple for the biological taxonomist as Joseph[4] has pointed out. The old logicians made it a little more inclusive by introducing the concepts of *subalternate genera* and *species infimae*, designating by this latter term the rank below which only *individual* differences can be discerned.

Aristotle used words corresponding to *genus* and *species*. We find them, for example in his work on the parts of animals, where man, lion, ox are referred to as separate species. He has also some cogent remarks about the way in which a classification should be set up.[5] But these various elements were not really brought together in a satisfactory way until the advent of Linnaeus who is regarded, by common consent, as the founder of modern systematics.

What great men do, often seems very easy after they have done it. Looking back from our standpoint on what Linnaeus did, it may not seem very remarkable. He took from the old *logica materialis* the concepts of genus, species and difference. He put every organism he dealt with in a definite species, designated by one single name, assigning it to a definite genus, also designated by one single name. The names he gave were Latin or Greek names, or names rendered into Latin or Greek. Thus Linnaeus endowed taxonomy with a specific language, or nomenclature, designating the fundamental elements with which it deals. For each species he stated the *differentia*, i.e. the specific characters. The combination of these reforms might have had little effect but for the energy and consistency with which Linnaeus applied them and but for his marvellous talents as a describer and classifier.

Some efforts have been made—for example by the late Dr. Walter Horn,[6]—to show, on the basis of a few *obiter dicta* that Linnaeus was in some sense a precursor of the evolutionary idea. But the whole tenor and trend of his work is against this. We have, to begin with, his celebrated aphorism: "Species tot sunt diversae, quot diversas formas ab initio creavit infinitum Ens": there as many different species, as the Infinite Being created at the origin of things. The clarity and concision of the descriptions of Linnaeus have often been noted. They bear witness, as it seems to me, to his conviction that there is in every living thing, something essential that the taxonomist should seek to grasp and record. His definitions are meant to apply, as the old logicians said *toto et solo definito*; and this is only possible if the object to be defined is really definite.

Linnaeus had, says Professor Maurice Caullery,[7] an Aristotelian and scholastic mind. When we consider his passion for exact definition, classification and logical deduction; and the 325 propositions of the Philosophia Botanica, fitting together as Caullery says, like the tubes of a telescope, we must agree. But when this author goes on to say that the mental outlook of Linnaeus led him to neglect the inductive method and attempt to impose on nature an order chosen *a priori*, we may venture to dissent. A taxonomist whose specific concepts have stood the test of time as well as those of Linnaeus cannot be fairly accused of neglecting the data of observation and the inductive method.

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Behind every scientific operation there are certain basic working principles. These are not always clearly discerned by the operator, as we can see from the earnest attempts the physicists have made in recent years to discover the meaning of the methods that have produced such remarkable results. But the tree is

[4]Encyl. Britt., 14th Ed., Vol. 18, p. 433.
[5]De Partibus Animalium, Bk. 1, Ch. 1-4.
[6]Gedanken über Entomologie. V. Int. Ent. Congress, II, Travaux, pp. 151 et seq. 1933.
[7]In Hist. de la Nation Française, Ed. Gabriel Hanotaux, T.XV, Vol. 2, 1924.

known by its fruits. The magnificent edifice erected by the disciples of Linnaeus is *prima facie* evidence that they are working along the right lines. But perhaps their working principles are not always clear to the systematists themselves. This is not meant as a criticism. Systematists learn rather by example and experience than by formal instruction. The good systematist develops what the medieval philosophers called a *habitus*, which is more than a habit and is better designated by its other name of *secunda natura*. Perhaps, like a tennis player or a musician, he works best when he does not get too introspective about what he is doing.

But this fact, if it is a fact, involves a danger: to wit, that sound, but unformulated principles may be undermined by unsound but formulated philosophies; and the whole purpose of this sketchy argument is to discuss, criticise, and if possible, refute certain philosophical doctrines or tendencies which now form part of our intellectual atmosphere but which could, if taken too seriously, destroy the foundations of rational systematic biology. Of these doctrines, the three that are to my mind the most important are idealism, nominalism and evolutionism.

There is no clear connection between these doctrines. They just happen to be components of the philosophical atmosphere of our time. But they all influence our thinking. I hope that by examining them critically the nature and value of the philosophical principles of systematics can be made clear and justified.

Idealism can best be understood in its opposition to realism. The philosophic realist says that something exists outside his mind and he can know what it is. The idealist maintains that he can know only what is in his mind. There is of course a certain ambiguity or inadequacy in speaking of what is "inside" and "outside" a mind, but it is understood that we are speaking only analogically.

The person mainly responsible for the success of philosophical idealism in modern times was René Descartes. On the 10th of November 1619, while meditating alone in a room in a village near Ulm on the Danube, Descartes received an illumination or revelation. He then perceived that while practically everything conceivable can be doubted, one final certainty exists, expressed by Descartes in the celebrated axiom, "Cogito, ergo sum": I think, therefore I am.[8] Why is this conclusion a certainty? Because, says Descartes, I *see clearly* that to think one must *be*. Thus the clear vision,—"l'idée claire"—is the ultimate test of this truth. Any true science must therefore be an integrated system of clear ideas. Descartes did not assert that the external world does not exist. But he did not believe that our sensory experience is a proof that it does. This sensory experience can only be accepted on the ground that God will not allow us to be completely deceived. Our basis for confidence in our senses is thus theological. To establish this theological basis, or, in other words, the existence of God, Descartes relied on what is called the "ontological argument", which, again, is a nexus of "clear ideas", requiring no data drawn from outside the mind. In spite of this reasoning Descartes did not really resolve his doubts about sensory knowledge for he says in the "Passions de l'Âme" that the "sentiments" called taste, odor, sound, heat, cold, light, colour, do not really represent anything existing outside our thought. On the other hand, Descartes was convinced that, starting with his "cogito", he could produce, by pure deduction, "the heavens, the stars, an earth, and even, on earth, water, iron, minerals" and a good many other things that are common and easy to apprehend.[9] The idealist Hegel, though he apparently did not claim to produce everything in the universe by a

[8]Discours de la Méthode, 4ième Partie.
[9]Discours de la Méthode, 6ième Partie.

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deduction from first principles, did, says Emile Meyerson, believe that he could deduce everything *essential*.[10] The joke about the German philosopher who sat in his study and deduced a camel from his inner consciousness is thus not merely a humourous exaggeration but does describe the idealist objective. The idealist enterprise as a scientific effort, is the negation of the basic procedure adopted by the systematist, who starts from direct observation and assumes its validity, holding that there is a world outside the mind that we can really know. The idealist position is adopted because it is held that the realist position is untenable.

But the idealist argument has been accepted much too readily. Not that anyone, even an idealist philosopher, really believes that the world is a projection of his mind. In practice this is not possible. But what is possible is to be infected by the idealist doubt: to be persuaded into a wrong methodological position by the contention that the existence of the external world cannot be *proved*. Assuredly we cannot prove this in the sense that we can prove one of the propositions of Euclid. In such proofs, we proceed by a process of combination and dissociation from known incontrovertible truths, to truths yet unknown. From what truth could we expect to deduce the existence of the external world? When Descartes says "I think, therefore I am", he is not really making a deduction. He claims merely that this is something he "clearly sees". His proof that the external world exists is based on the idea that God will not allow us to be deceived. But his proof of the existence of God is itself fallacious. The whole history of modern philosophy shows that once we have locked ourselves up in the idealist prison it is impossible to get out; I mean, it is impossible to pass from thoughts to things.

But although it is possible to assert a doubt that the external world exists there is no reason to entertain it. We say the external world exists, not because we can deduce this from some previously known truth but because it is *evident*. The case is in fact similar to that of such propositions as: what is, is, or $1+1=2$. These are not susceptible of proof but do not require proof. Their truth is immediately apparent. We cannot agree with Descartes that the test of truth is the clear idea. An idea may be clear and yet false. Nevertheless, the absolutely primordial truths are *seen* though not proved because their contraries are clearly unthinkable. So it is also, *mutatis mutandis*, in the sensory order. The eye that sees cannot simultaneously deny, so to speak, that it sees, because this would simply mean not seeing. To put that matter in another way, if a surgeon operates on a blind man and *makes* him see, he cannot and need not also *prove* to him that he sees.

The Aristotelian realists of the middle ages, who dismissed as mere folly the suggestion that the external world does not exist, were well aware that for various reasons we may not perceive things as they are. They distinguished between what they called "sensibilia propria"—that is, the specific objects of the various senses, such as colour and sound, and the "sensibilia in commune", such as size and shape, which are known to us through the "sensibilia propria" and by utilising several senses. They readily admitted the possibility of erroneous judgments in regard to the "sensibilia in commune" but they held that a sense-organ is not deceived with respect to its proper object. This question requires more consideration than we can give it here but it may suffice to say that the very fact that we speak of errors in sense perception is a proof that we believe true sense perception is possible. It is obvious also that since all the scientific instruments we utilize in investigating nature are merely in one way or another,

[10]La Deduction Relativiste, No. 124 etc. 1925.

extensions of our senses, radical scepticism in regard to sensory perception would completely undermine science.

My first conclusion therefore is, that philosophic realism, which is a fundamental working principle in biological systematics is not a naive and outmoded relic but is perfectly defensible on rational grounds. The systematist is describing and analysing a world that really exists.

At the opposite pole to idealism but, as it seems to me, as destructive in its way to the status of systematics as a science, is the doctrine of nominalism which is attached to the name of the 14th century philosopher William of Ockham. William of Ockham is justly regarded as one of the precursors of the scientific movement, though he made no personal contributions to science.

For Ockham,[11] in contrast to Descartes, the only certain propositions concern what is immediately evident and what is immediately evident is what is revealed to us by the senses. What we apprehend through sensory perceptions is the concrete singular- the individual- and, therefore, the only certain knowledge we have, relates to concrete individuals. Nevertheless our science is composed of propositions to which we attribute a general value. Science, we commonly say, consists of generalizations. The definition of a species, in systematics, is such a generalization. What then can be its value or significance? When we frame the specific definition of *Musca domestica*, to what does this definition refer? The answer of Ockham is that it refers to a concept in which the individuals are *confused*, by which he means, that the individuals are not distinguished one from the other. If, considering the individual, we call it *the* House Fly, that means that we designate only the concept, corresponding to a *confused impression.* Only *this individual house fly* is known distinctly and it really should have a name like John Smith, indicating its unique individuality. If now we ask are not all House Flies in some respect *the same*, the answer of Ockham, formulated in the most positive and rigorous manner is *no*! There are no doubt *resemblances* between these various individuals but the *only* thing that unites them—the only thing that makes them in a sense the same, is that we have given them *the same name.* The name itself is an *individual* thing and its *universality* comes only from the *application* of it to a series of individuals which, nevertheless are not, *in themselves, in any sense the same.* This means that the species has no basis in reality. Mr. de la Torre-Bueno has said[12] that either the species is a concrete reality or else it is a figment of the imagination. If it is a concrete reality, we can identify it only in a single individual. But it is not, according to Ockham, precisely a figment of the imagination; it is rather a cloudy and confused *image* of different individuals, lumped under the same name. You can see why this solution of the problem of the universals is known as *nominalism.*

The last step in this development is to be found in the views of authors like the logical positivist Gilmour in "The New Systematics".[13] It should never be forgotten, says Gilmour, that the *individual* is a concept, a rational construction from *sense-data* and that the latter are the real objective material of classification. Though William of Ockham may be regarded as the intellectual ancestor of this conclusion, it would probably have surprised him. He was convinced that the individual—something which is, as the medievals put it, distinct from everything else but indistinct within itself—was a fundamental and primary datum. What altered the position was in the first place the revival of atomism, making the material individual difficult to delimit and in the second place the explanatory system of mathematical physics.

11cf. E. Gilson, La Philosophie au Moyen Age, Ch. IX, 3, 4, pp. 638-687. 1944.
12Ann. Ent. Soc. America, Vol. XXXIV, Vol. 2, p. 287. 1941.
13l.c., pp. 461-474. 1940.

William of Ockham by his restrictions reduced the system of classification to something entirely empirical but also entirely arbitrary. Gilmour asserts that the object we call a *chair* consists partly of a number of experienced sense-data such as shapes, colours and other qualities and partly of the concept *chair* which reason has constructed to "clip" these data together. Sense data, he says, are given, whereas "clips" can be created and abolished at will.

This statement, taken at its face value, makes our system of classification arbitrary to a degree that even William of Ockham did not anticipate. Taken literally, it means that I can decompose the sensory data given by the furniture in this room and recompose them so as to make a landscape; and this result, consisting of the sensory data connected by the "clip" called "landscape" will be just as valid as the assemblage with which I started. If you think this is not meant to be taken seriously, you are wrong. Large books, filled with subtle argument have been written to support it. However, a study of these works suggests that the idea is not quite so absurd as the quotations given suggest. It can be interpreted in a comprehensible fashion though whether its supporters would sanction the interpretation is uncertain. The operative word, in Gilmour's statement, I take to be the word "rational". Our justification for connecting a certain set of sense-data with the concept or clip called "chair" is that the connection is *rational.*

Now if we had never seen or heard of a chair, we certainly could not give a completely rational account of it because we would not know why it is, as it is. It would thus appear to us as a *confusion.* The knowledge that it is made to sit on would enable us to *understand* its structure and form as related to a certain end. If we observed that it was used simply as a coat-hanger or as an ornament, we might consider its form and structure in relation to these ends. In these several interpretations we would pay attention chiefly to the features of the object that seem to be related to the end we have in mind. Other features we would regard as incidental or accidental.

However, the possibility of connecting the sense data provided by the chair by rational "clips" of various kinds is, after all, strictly limited. It is limited by the fact that the chair is actually a definite individual *something* having properties assembled in a certain manner, independently of our interpretation of its function. It is limited also by the fact that the chair is a realization of an idea formed by the carpenter who built it. It has therefore, not only a material unity, due to nails and glue, but a teleological unity due to the fact that it has been designed for a definite purpose.

If these limitations are not admitted, then it must be pointed out that the sensory data themselves are individualized and their individuality can also be regarded as the result of applying a conceptual clip. The black square seat of the chair has no better entitative status than the chair itself. The sensory data thus become things which we can vary in a manner that will permit the construction of an indefinite variety of objects by means of conceptual clips.

That anyone would seriously propose a theory of knowledge that would make our classifications completely arbitrary seems unlikely. On the other hand, the rational interpretation of sensory data in relation to function is a commonplace, as old as Aristotle. In modern times it was magnificently developed by Cuvier. It seems clear that the biological positivists of today have something else in mind.

What they are attempting, I think, is an application of the methodology of mathematical physics to biological systematics. This view is reinforced by the remarks of the positivists about definitions. The meaning of a statement, they

say, is defined by the method of verifying it. This is quite intelligible with reference to statements in physical science. The meaning of the statement: the air is humid, is that such and such readings are obtained on the wet and dry bulb thermometers. The meaning of the statement: an electric current is passing through this wire, is that the needle on the voltmeter has moved. All this suggests that in the system of logical positivism, sense-contents are dealt with as in mathematical physics. It seems that to define the meaning of the statements: this is a Carabid; or this is a Red Pine, in this manner would require some pretty involved verbal acrobatics. However this may be, the standard practice in mathematical physics is to start with a mental construct, representing the entity that is being studied. This construct is assumed to be regulated by certain laws. If now the mental construct, operating according to the laws postulated, gives results similar to those observed in the natural entity or situation it represents, then the mental construct may be considered to be in some sense, a correct representation of the natural situation or entity that is being investigated. A very simple and ancient example is the solar system. The objective here was to produce a mental construct operating in a manner that would place the heavenly bodies at all times in the positions in which they are actually seen. The Ptolemaic system which placed the earth in the centre of the universe, was created with this object. The medieval thinkers who considered the matter realized and definitely said[14] that some other system that would work as well or better, might eventually displace the Ptolemaic system. Increase in the accuracy of measurements with the resulting complications eventually led to the rejection of the hypothesis but it was not until quite recent times that it became possible to select the Copernican system to the exclusion of all others.[15] Here we arrive at an explanation of the observable facts by constructing a pattern which is not itself observable: since we cannot see the solar system as a whole. This pattern assembles the sensory data in a rational construct but since the natural pattern eludes observation the construct is liable to be displaced by another, agreeing better with more exact observations. Similar methods are applied to physical phenomena by constructing patterns of which the elements are completely unobservable. Their entitative status, since it cannot be ascertained by direct observation, is guaranteed only by the fact that deductions made from the hypothesis in which they are involved, coincide, within the limits of experimental error, with measurements made in nature. No one can refuse to admit the immense value and fruitfulness of this method, particularly in the physical sciences. Nevertheless, it must be remembered that it does not provide the certainty attached to direct observation, while on the other hand the elementary data it is intended to explain, like the data used in verification are themselves directly known and the object of sensory intuition. Immensely valuable as the hypothetico-deductive method is in its proper sphere, it is not the only nor the most reliable method of discovering the truth about nature and its success in its own field in no way alters or diminishes the status of the basic material handled by the systematic biologist. The individual organism, object of our direct sensory intuition remains the elementary unit in the science of systematics.

On the other hand, so much being admitted, we have still to establish, as against the nominalist criticism of William of Ockham, the normal view of the systematic categories; and first of all of the category of the species.

Consider first the generic and specific names, which, taken together, designate, we say, the species. The names are words. Words are signs. The essence of the sign is reference to something other than itself. Words are material signs:

[14]cf. Aquinas. Summa Theologica, I. Q. 32, art. 1, ad 2.
[15]cf. F. R. Johnson. Astronomical Thought in Renaissance England, p. 106. 1937.

that is, they have, as William of Ockham pointed out, their own individual entity in addition to their entity as references. Concepts are also signs but they are signs whose entire entity consists in the reference to the thing signified, for the concept is simply *that in which we know the thing*. Words refer to things, not directly, but by the intermediary of the concept.

In systematic biology we place, between the name and the concept,—since the name itself, though often descriptive, is inadequately descriptive;—the *definition* of the species. To what does this definition refer?

What sensory intuition gives us, is the concrete individual: this particular specimen with its individual characteristics. If this concrete individual is the object of the specific definition—and this is really what we mean when we say that a species is something *concrete*—then species and individual mean the same thing and classification is impossible since classification means the assemblage of individuals into classes, themselves susceptible of definition. It is clear, therefore, that the concrete individual is not the species.

Another possibility would be to say that a certain collection of individuals is the species. This involves several difficulties. These individuals, being individuals, differ in their positions in space and time and probably in their intrinsic characteristics. If nevertheless they *are* the species, the specific definition must define it in contradictory terms, as having something and also not having it. Furthermore if really a number of individuals *is* the species, we must ask how many? Because every collectivity is numbered and its number is its distinguishing characteristic. Again, the collectivity is not a unit but a collection of units. A collectivity, therefore, cannot be *the species*.

Since the species is not the individual, nor the collectivity of individuals, it does not exist, as such, in the real world. It can therefore, exist as such only in the mind.

The problem of biological species can be seen more clearly, when we realize that it is merely an aspect of the general problem of classification. When we define copper sulphate or sodium or a geometrical figure, or a house, we are defining species and the relation we are trying to understand is much the same. If by nature or essence, we mean simply, what a thing is, then we must say that the nature of this piece of copper sulphate is what it individually is and the nature of that piece is also what it individually is; and yet it is true to say of both that they are copper sulphate. Similarly every individual triangle no matter what its individual characteristics or as we might say, its individual nature, is truly a triangle. Thus the triangle, as the object of the specific definition, exists only in the mind; but in every existing triangle it is individualized. The object of the specific definition is therefore an abstraction which has nevertheless a foundation in the individual natures. It is not therefore a figment of the imagination, neither is it a pure idea having no relation to the material world. It is an abstraction having nevertheless a foundation or source in nature since it is realized in the individuals representing the species. These truly have something in common; and this is the object of the specific definition:—

When we say the object of the specific definition has a foundation in nature we must not be seized by the delusion that it exists as such in each separate individual of the species. This was indeed the view of the medieval philosopher, Duns Scotus, who considered that the specific essence is present in each individual but affected by a determination he called "thisness" which gives it its individuality.[16] No, the essence is individual, not a specific essence overlaid with individuality like the skin over an orange. Nevertheless the individuals have a simili-

[16]cf. Gilson, l.c., Ch. IX, 1.

tude which the abstractive intelligence can seize and which is the object of the specific definition. In every triangular object that exists in the real world we can truly recognize triangularity; but pure triangularity or the pure unparticularized triangle, exists only in the mind.

A difficulty may be suggested at this point, connected with the fact that systematists usually select one individual as the *type* of a species and sometimes describe a species from a single specimen, having no others. However, this is rather a matter of strategy than of principle. Modern work shows that the specific definition, if based on adequate material, represents any particular character as varying according to certain definite laws so that we can say what chance there is that it will have a certain value, but cannot ascribe a definite value to it. Thus the species, as given by the specific definition is what we might call a zone of probability, which is, nevertheless, not a population, but a unit. Nevertheless we have to examine a population to obtain the definition. This simply exemplifies the truth that the species is a class or category, one in the mind but individualized in nature in the members of a multiplicity.

I should like now to consider briefly the question of evolutionary theory in relation to biological systematics. Since natural science attempts to explain the material world in terms of natural causes, the theory of evolution appears to have a sound methodological basis. Nevertheless, the introduction of evolutionary concepts into systematics has produced confusion in regard to certain matters on which the pre-evolutionary systematists were quite clear.

Let us note, to begin with that it is very difficult to reconcile evolutionary theory and systematic method.

This difficulty is due to the fact that when all is said and done, the theory of evolution presents the organic world to us as a continuum whereas biological systematics presents it as a system of discrete entities, comparable to the elements and compounds of the inorganic world. For the evolutionist, the organic world is like a sea of protoplasm, which pushes out arms and bays and differs superficially in the various parts of its vast area but is fundamentally the same. The evolutionist's preoccupation has therefore always been to connect the taxonomic units, to argue away the differences between the categories; while the systematists' effort has been to depict the objective reality. A subjective element enters his work by way of his judgment; but his basic assumption is that his method enables him to get nearer and nearer to an exact transcription of what actually exists. What he has produced, as we know, is a hierarchical but discontinuous picture. It is clear, of course, that systematics is based on definitions and that any attempt to found it on an assumption that the things of nature are indefinable, is self-destructive. One of my systematic friends asked me one day whether, in saying that species are abstractions, I mean that it abstracts the specific definition from the evolutionary movement: at least, this is how I understood the question. The answer, I think, depends on whether we are dealing with something that *is changing* or with something that *is only change*. In the first case we have an essential something that persists and this is what we should try to grasp if we wish to produce scientific propositions of permanent value. In such a case we can abstract from time and space. However, if we are dealing with what is essentially change, as in the evolutionary philosophy of Henri Bergson, then abstraction is falsification, as Bergson indeed maintained,[17] because if we try to abstract from change by eliminating change we are left with nothing. As Williston once put it in a curious phrase, "Evolution never classified anything".

[17]cf. L'Evolution Créatrice.

The effort to adhere to evolutionary principles has engendered some strange fallacies.

The first is, that classification is and indeed must be phylogenetic. Only a phylogenetic classification, it is said, is a *natural* classification. We readily admit that the individuals of a species descend from a common specific ancestor. Generalizing, we say, that the species of a genus A descend from a common generic ancestor different from that of the species in the genus B. But if the genera A and B are more similar to each other than they are to the genera C and D which are morphologically similar, then we may say that their generic ancestors are different, though they may have a common tribal ancestor. In establishing these relationships, that is to say, these phylogenetic connections, we rely on homologies. Homologies, says evolutionary theory, indicate genealogical relationship, while analogies do not. According to a recent evolutionary theorist, the pre-Darwinian anatomists, when they distinguished between homologous and analogous organs were, in reality, by a subtle intuition, whose significance they themselves did not appreciate, distinguishing between parts connected by descent or genetic relationship, and new formations. We have therefore no need of fossils to demonstrate evolution. "The community of descent" said Darwin in the Origin of Species, "is the hidden bond which naturalists have been unconsciously seeking". "I believe" he continues "that the *arrangement* of the groups within each class, in due subordination and relation to the other groups, must be strictly genealogical in order to be natural".

Now the status of a hypothesis in science depends on the possibility that it can be *verified*. It is absolutely certain that the practical verification of this hypothesis—and it is only a hypothesis—is impossible. In a family like the Braconidae, the systematists can readily recognize well-defined clearly separated sub-families and genera. Is this a "natural" classification? In any case it is one that cannot possibly be justified on phylogenetic grounds simply because we know nothing about the origin of the species of the Braconids. The same thing applies to the detailed classification of all groups. It cannot be based on phylogenetic data because such data do not exist. What the evolutionist does, is to deduce phylogeny from classification, not classification from phylogeny.

That classification must be based on phylogeny is easily disproved. Mendeleef's table of the elements is a classification. The bromides and the sulphides constitute, each in its way, definite and recognizable groups. No phylogenetic considerations can be introduced here.

The idea that homology is based on and determined by phylogeny, is another unverified, unverifiable and unnecessary hypothesis. The practical absurdity of this can readily be demonstrated. Suppose I have before me specimens of two species of the Tachinid genus *Exorista*. In each, I recognize what I call the radio-medial cross-vein. What can we say about the relation between the two cross-veins in the two species? Since we are dealing with two different concrete material entities, we cannot say the cross-veins are the *same*. Yet in a sense we identify them. We say, in fact that they are homologous. But have we demonstrated this by showing that the two individuals, or even the two species, descend from a common ancestor? Certainly not, and, what is more, we have not really the slightest expectation that we can ever do this. Supposing that we have this expectation, the hard fact remains that it has not been realized. A realistic survey of the position therefore indicates that the establishment of homology based on phylogeny is untenable and if this is its only basis, comparative anatomy must be relegated to the scrap-heap. But this is a very simple matter. Homology has a geometrical foundation. It is based on identity

of spatial relationship. The angles opposite the base of a series of triangles are homologous angles.

The idea that homologous organs indicate genealogical relationship, while analogous organs do not, rests on the idea that the homologous organs are original and ancestral while analogous organs are new developments, grafted, so to speak on an original basic plan. For all we know, analogous organs may have arisen as early as homologous organs. Who can say that the fore limb of the bat was ever anything but a wing?

Professor Ferris says[18] that homology indicates community of origin. With this we may agree. But the words "community" and "origin" are ambiguous. They may signify either descent from the same ancestors or material constitutions specifically the same. Two Echinoderm eggs are spherical. Is this because they are eggs of the same Echinoderm? In a sense, this is so. But a more interesting and fruitful explanation is that they are both physically homogeneous liquid drops whose substance has arranged itself according to well-known physical laws, so that its surface is one of which the potential energy is a minimum.

The general tenor of the evolutionist argument suggests that the interpretation is based on the assumption that the species is the ancestor of the individuals, the genus the ancestor of the species, the family the ancestor of the genera. In reality, these categories are merely what the medievals called "metaphysical degrees". Like the species, they are abstractions, each category having a greater *extension*, as the logicians say, than those below it, while on the other hand, it is less *comprehensive*. Thus the category Hymenoptera is more extensive than the categories Ichneumonidae and Braconidae, since it includes both, in so far as they have something in common; but it is less comprehensive, since it excludes what is special to each group. Since this category is only an abstraction of the second or third degree, it obviously cannot function as an ancestor. For example it has wings of no definite venation, an ovipositor of no definite form, amorphous mandibles. No such animal can exist, nor can it even be imagined.

To determine whether a classification is "natural" the ancestry must be determined and to follow the ancestry down the original ancestor must be known. Some workers consider that by the examination of the organisms existing at the present time, this historical problem can be solved. "An examination of the structure and development of the most primitive representatives of the class", wrote Dr. A. D. Imms, in his fine "Text Book of Entomology", "renders it possible to construct the archetype or ancestral form of winged insect".[19]

It is not necessary to repeat here the description of the ancestral insect given by Dr. Imms. The point I wish to make is that if we can take this ancestral form we can produce various existing forms from it by the addition, modification and subtraction of parts. If we can place species existing today along the lines of ancestry thus established, the arrangement may be described as a phylogenetic or "natural" classification. Thus, to take only one example, let us suppose that in the primitive 4-winged insect, the posterior pair of wings undergoes a reduction and modification so that it becomes a small club-shaped organ with highly developed sensoria; that the network of thickenings postulated between the veins of the primitive wing disappears, with the 4th medial and first anal veins; that there is a modification of mouth parts leading to the production of a suctorial organ, replacing the original opposable mandibles; and finally, that there is a reduction in the number of the abdominal segments. We shall then have an insect corresponding fairly well to the present day crane-fly. If we now imagine

[18]The Principles of Systematic Entomology, p. 63. 1928.
[19]l.c., 3rd Ed., p. 4, 1934.

a reduction in the length of the antennae with a progressive fusion of segments and modification of those which remain, together with a reduction in wing venation and various minor modifications, we can transform the "primitive Dipteron" into a graded series resembling the members of the actual families in this Order. On the basis of operations of this kind it may be claimed and biologists do often claim, that the classification of existing forms they have duplicated is a natural or phylogenetic classification.

In reality, of course, the process is nothing but an ingenious exercise in solid geometry. The so-called ancestral insect is simply a mental construct. That we can produce from this, by modifying it in various ways, all the forms whose descent we are investigating is in no way surprising because it has been built for this purpose. It is clear that in such cases the "reconstructions" of phylogeny are entirely vain and have not, intrinsically, any relation whatever to historical reality. The biological prestidigitator is simply taking out of the hat the rabbits he put into it. The method used in working out the descent is in fact a perfect example of the argument in a circle.

This is not to say that the actual work done in these cases is without value. It is all a question of considering it in the proper perspective. To regard it as a real reconstruction of phylogeny is simply a childish delusion. But as a development of comparative anatomy it is quite legitimate. Work of the same general character was carried out by the pre-evolutionary anatomists and if the evolutionary theory is some day relegated to the scientific dust-heap, it will still go on. The camouflage of evolutionary verbiage applied to the process does not alter its fundamental character.

Supposing we could really prove that the species of an existing genus all descend from one original ancestor we would then certainly have a fact of considerable interest. But it is idle to speculate on what it would mean to us, since we do not have it. The phylogenetic reconstructions I have been discussing are valueless as what they purport to be, because there is no way in which they can be verified.

Let us take as an example, the parasitic Diptera of the family Tachinidae. According to the late Dr. C. H. T. Townsend, the age of the Tachinid stock is about sixty million years. The oldest known fossil is *Vinculomusca vinculata* Scudder from the Paleocene of the Chagrin Valley, Colorado. Townsend lists only 17 genera to the end of the Miocene that can be referred with certainty to the Oestromuscaria, which includes the Tachinidae. He gives the descriptions of all of these flies in his "Manual of Myiology".[20] Not one is sufficiently accurate and detailed to enable the genus to be identified with a modern key to the family; but the condition of the specimens probably would not permit more accurate descriptions. Since practically nothing is known about the fossil predecessors of the Tachinids, it is clear that nothing can be said about their actual ancestry; yet statements about the evolution and phylogenetic rank of Tachinid species and genera are freely made by some of the authorities on the subject.

Owing to the passionate desire of biologists to reconstruct phylogenies, no fossil is considered to have played its part in palaeontological science until it has appeared as an ancestor. In other words the procedure followed has been rather to reconstruct phylogeny with the materials available, rather than to wait until sufficient material has accumulated so that it has reconstructed itself by the formation of an obvious morphological continuum. This procedure is evidently fraught with great danger. If the ancestors of a bisexual species double at each generation, the number of "grandparents" in only 100 generations becomes

[20] l.c., Pt. XII, Sect. XXVIII, 1942.

almost astronomical. Of course the actual number of *different* ancestors is far less, because each separate individual gives rise to so many lines that many ancestral series lead back to the same ancestors. Nevertheless when we consider the fact that over 1400 species of Tachinidae, distributed in several hundred genera, exist today in North America, the impossibility of following the ancestry of any one through the inextricable tangle of forms preceding it in geological history must be unhesitatingly admitted; and would remain, even if the predecessors were well represented by fossils.

When we reflect on this point, we feel bound to ask ourselves where we get the curious habit of talking about *the* ancestor of the Diptera or of the Leguminosae? I suspect that this is a simple anthropomorphism which derives from the fact that in considering human ancestry, genealogists have the habit of following it only in the male line. If I claim that I belong to an "old" family, I am required to prove it by producing an attested genealogy composed only of men named Thompson. Obviously, ancestry in the biological sense means something entirely different.

It is often said that the evolutionary system has, at least, "illuminated" the taxonomic system, which would not, without this guiding principle, have been able to acquire the essential rationality it now has. "Outside of the Evolutionary hypothesis" says Professor Maurice Caullery, "there can be only artificial classifications, founded on the arbitrary choice of certain characters".[21]

This position is untenable. If the arrangement of species in the system of classification can only be decided by establishing the degree of genetic relationships then almost the entire systematic structure must be abandoned, because the genetic relationship of species cannot in general, be determined. If the arrangement of species can be decided in some other manner, then the establishment of the genetic relationship is not necessary. In most groups nothing whatever is known about genetic relationships and it is most unlikely that positive and sufficient information can be obtained. In such cases we should have to say, on Professor Caullery's postulates, that one classification is as good as another, which all biologists would certainly regard as absurd; and which manifestly contradicts our experience. We must therefore conclude in the first place that classification cannot possibly be phylogenetic and in the second place that it need not be.

It is clear therefore that classification must be based on the observable attributes of existing organisms as they are here and now. These attributes fall into two classes: the material and the functional. We rely, in establishing classifications, on the material, rather than on the functional attributes: on morphology, rather than on physiology or behaviour. One reason we do this is simply that we normally have to work on dead material. Material attributes persist after functional attributes disappear. We therefore tend to think of function as a derivative of form and the content of our specific definition is therefore morphological. The systematist is looking for the building stones of the organic world. Perhaps he is not anti-evolutionist, but at least he is non-evolutionist; as much as a "fixist", so to speak, as Linnaeus. Where, if not in the material attributes of material things, can he find the permanence that he requires to found his science?

But if we look into this matter a little more closely, we can see that the ideal of the systematist cannot be fully realized in a collocation of material properties, considered as such. The modern work on heredity and variation, though it has not eliminated specific distinctions, as some evolutionary theorists

[21]Le problème de l'Evolution, p. 110. 1931.

anticipated, has certainly made the difficulty involved in a morphological definition of species, more apparent. The systematic ideal is the definition of the species as a *true entity* in which the elements recognized cohere, so to speak, in virtue of a *visible necessity*. So long as the notes in the specific definition constitute only a mere aggregate, this ideal has not been attained. I suspect Linnaeus did not altogether realize this because he thought rather as a logician than as a philosopher. But to Cuvier, perhaps the greatest of the comparative anatomists, it was clearly apparent. In a well-known passage in his "Discours sur les Revolutions du Globe", Cuvier defined, with Gallic clarity, what he saw as the basic philosophical principle of rational anatomy and, therefore, of rational systematics. "Every organized being", he said, forms a *whole*, an unique and closed system, whose parts mutually correspond and contribute to the same ultimate action by reciprocal reaction. None of these parts can change unless the others also change, and, consequently, every one of them taken separately indicates and implies all the others"; and he went on to apply this principle to the anatomy of a carnivorous animal. He was convinced that the form is conditioned by function; and that the relation between form and function is governed by laws as inevitable as the laws of metaphysics and mathematics. Interpreted in this way, the specific type acquires the coherence and necessity of a geometrical definition from which we can deduce all the attributes of the object of the definition.

Looking at the matter from another angle, we can regard the organism, with the physico-mathematicians, as a dynamic unit or field of force in which morphology is the expression of physical laws whose operation we recognize in the form of a liquid drop or a wave, or an aggregate of soap bubbles.[22]

However, the accumulation of material has been too rapid and the opportunity for studies in behaviour, physiology and morphogenesis, too meagre, to allow us to progress very far along these lines. When all is said and done, morphology in itself remains the working basis of systematics. On a morphological basis, illuminated only in certain favorable cases by other considerations, it builds up the record of types, relations and hierarchies in which the intrinsic order and the fundamental constancy reveal to us, though only "through a glass darkly" the existence of laws we cannot completely grasp and define because we apprehend them only through empirical or observed connections.

Nevertheless, the systematic structure is essentially sound. If you pay too much attention to the conversation of the systematists you may be led to doubt this. They wonder whether the external world exists, they suspect that our senses deceive us. The organic species, they will tell you, when in an evolutionistic mood, is as difficult to isolate and define as the phases in the flight of an arrow or the eddies in a running stream.

In spite of such professions, the systematists have erected, at the cost of incredibly painstaking labour, on a methodological basis of philosophic realism, an enduring monument, in which the essential order of the organic world is revealed: a monument, as the Latin poet says,[23] *aere perennius*—more enduring than bronze. This is the central core and framework of biology.

[22]In spite of the wonderful advances that have been made in this field, I doubt that the systematist can thus define the necessary entity he is attempting to delimit.

[23]Horace, Carmina, III, 30, 1.

Dignitaries: A.W. Baker, J.M. Swaine, Howard Trueman, W.E. van Steenburgh, H. Martin.

J.G. Oughton, A.V. Mitchener, H.R. Boyce & S.E. Dixon chat.

F.M. Cannon & E.M. DuPorte paused for a lensful.

R.W. Salt, C.J.S. Fox and A.W.J. Dyck caught a flash for a celluloid close-up.

Cyril Copeland & R.W. Fisher were on hand.

H. James listens.

G. Wishart, W.C. McGuffin, H. Wong, R.J. Heron and M.G. Maw took the cameraman seriously.

Ted Armand, L.A. Roadhouse, Sam Loschiavo, O. ...eck, S. Walley and Misses E. Arnott, Freda ...rbyshire and R. Derraugh get into the act.

John Sharpe & Dave Gray look sharp.

Mrs. Taggart and her Deputy husband enjoyed the banquet.

Dr. J.G. Taggart, Deputy Minister, was in fine form.

S.D. Hicks, W. Haliburton, G.G. Dustan, Bob Forbes, Fred Cummings & John Arnold, lobbyin

C.G. MacNay, Tom Freeman and Paul Perron look like secret agents, have that arresting look.

H.H.J. Nesbit gave a mitey good paper.

E.B. Watson, H.A. Butler, Barbara Michael, G.S. Fligh & L.A. Miller - imposing group.

President Ross, looking preacherwise, led the congregation. Let us spray.

C.E. bro good wis rom Cor .

W.R. Thompson waxed philosophical.

A.S. West, B.N. Smallman and Mrs. Smallman look camerawise, distinguished.

Discussion Period: E. Monroe had a word to say re systematics.

W.E. van Steenburgh holds breath while Rev. Fourni pears unsuspecting as Pe er lets go with a couple

ov. President
N. Keenan -
ecutive type.

J.L. Auclair used the chalk-talk method.

G.P. Holland let his hair down systematically, accordion to Dick Painter (left).

A.D. Pickett picked a good subject.

OTTAWA
November
1-2-3
1951

W.J. Brown makes a good interest study.

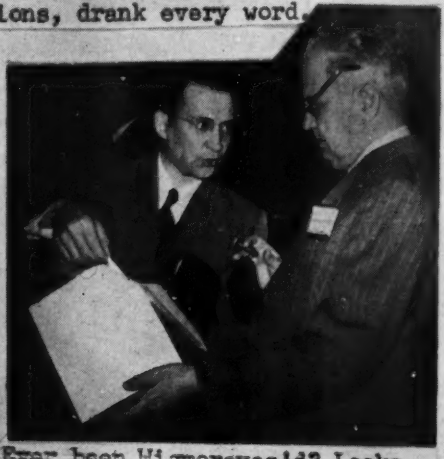
Prof. G.J. Spencer mimics grasshopper, had audience in stitches.

John Stanley enjoyed banquet as did Jim Marshall (background).

Prof. E.M. Walker lent distinction to the sessions, drank every word.

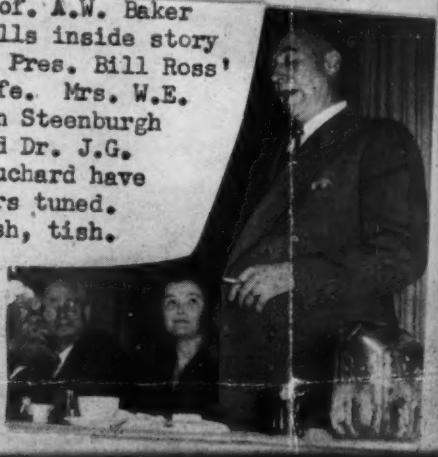
J.W. MacB. Cameron put a plug in for the Soo Lab.

Scientific Study: A.W.A. Brown gets advice from the old master J.J. de Gryse, daddy-of-em-all.



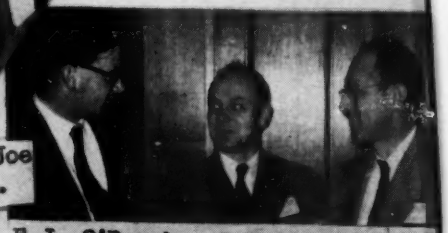
Ever been Wigmoreyes'd? Looks like President Ross is getting the treatment. Be careful, Bill.

Prof. A.W. Baker tells inside story of Pres. Bill Ross' life. Mrs. W.E. van Steenburgh and Dr. J.G. Bouchard have ears tuned. Tish, tish.



Registration Time: H. Hemming, Mr. X., Joe Duncan, F.M. Cannon and K.M. King pay up.

W.A. Fowler, L.G. Roadhouse and G.M. Davies got in way of camera.



F.J. O'Rourke, Bob Glen and H.H. Ross (U.S.A.) exchange ideas.

J.G. Rempel from Sask.

Hubert Martin relaxes.

M.M. MacOdrum, Carleton College Head, puffs away.

G.P. Holland, J. Stanley, E. Monroe and B.P. Beirne engage in a round of bugology bugaboo

Big Three: G.B. Oakland, H.E. Gray and F.L. Watters.

Choir Boys: L. St. Laurent, Paul Beaudoin, Andre Beaulieu, Paul Perron, Joe Duncan whoop it up -- to mention a few.

H.A.U. Monro looks over L.G. Putnam's shoulder.

H.R. Boyce, R. Beard, J.P. Johns, Jack Jakiey, M. Zappe and P. Garman - big shots!

Mrs. C.W. Parsons charming profile.

Descriptions of Several New Species of Caddis Flies

By D. G. Denning

In this paper two new species of Chimarra (Philoptamidae) and five new species of Hydropsychidae are described. The majority of the new species are from western United States. Grateful acknowledgment is made to Dr. R. H. Beamer of the University of Kansas for sending me many of the specimens used in this paper. Unless otherwise designated types of the new species are in the collection of the author.

Chimarra adella n. sp.

The peculiar apico-ventral flattened projection of the aedeagus and the distal margin of the tenth tergite will easily distinguish this species from others described in the genus.

Male. Length 6 mm. Head, body and appendages uniformly black, tibia and tarsi only slightly lighter in color. Wings blackish throughout. Antennae and palpi are missing.

Genitalia as in fig. 1. Ninth segment annular, ventral portion wide, its margin rounded and not projecting much beyond distal margin which is fairly straight, dorsal portion gradually constricted to a narrow band. Claspers with base narrow, ventral corner extending caudad beyond remainder, dorsal corner sharply differentiated as a narrowed finger-like process which continues mesad as a narrow thin blade-like process when viewed dorsally, apical margin serrate; convex outer surface with setae sparse, inner surface when seen from caudal aspect, fig. 1a, concave, dorso-apical process finger-like and directed mesad. Tenth tergite lightly sclerotized, mesal portion semi-membranous; apical margin from lateral aspect with a rounded dorsal portion extending dorsad of remainder, about midway a triangularly acute projection directed ventrad, ventral portion narrowed, rounded and projecting below remainder; from dorsal aspect, fig. 1b, apico-lateral corner developed into a prominent apically acute process directed laterad. Tubular portion of aedeagus comparatively short, ventral margin projected beyond remainder as a flattened, long, triangular portion, fig. 1c, bearing a considerable number of ventrally located short flat spinules.

Holotype.—Male; Cochise Co., Arizona, Chiricahua Mts., July 20, 1950; (R. H. Beamer).

The holotype is deposited in the entomological collection of the University of Kansas, Lawrence, Kansas.

Chimarra xesta n. sp.

The distinctive claspers with a circular incision along the inner margin together with the tenth tergite will easily distinguish this species from other Chimarra.

Male. Length 7mm. Head, body, palpi and legs except tarsi light yellow in color, tarsi light brown; pubescence of head and thorax sparse, golden colored. Antennae and wings brownish; spurs 1-4-4.

Genitalia as in fig. 2. Ninth segment with dorsal margin somewhat truncate and much narrower than ventral portion which extends caudad as a triangular projection. Claspers with base narrow, from lateral aspect ventral margin widely rounded, dorso-apical corner truncate; seen from dorsal aspect apico-dorsal margin projected mesad as a rather narrow, acute process; from caudo-ventral aspect, fig. 2a, margin of the slightly concave inner surface with a prominent circular incision; setation relatively sparse. Tenth tergite with lateral plates somewhat quadrangular, lightly sclerotized, apical margin rounded, from dorsal

aspect, fig. 2b, lateral margin with a sub-triangular process about midway and an acute triangular projection apically. Near junction of ninth segment and tenth tergite the inconspicuous rounded cercus is located. Aedeagus cylindrical, containing two prominent blackish spines internally.

Holotype.—Male; Cuernavaca, Morelos, Mexico; February 4, 1947; (N. L. H. Krauss).

***Homoplectra oaklandensis* (Ling).**

In 1938 Shao-Win Ling described a single male from Oakland, California as *Homoplectra oaklandensis*. The type, which is badly mutilated, was kindly loaned to the writer by Dr. E. S. Ross. The drawing of the male genitalia, fig. 3, was made from the type.

Distinctive characters in the armature of the aedeagus are as follows: (1), a dorsal pair of acuminate, slender, distally acute arms; (2), a laterally located spur, arcuate, acuminate, and directed caudodorsad; (3), the aedeagus proper is elongate and tubular; (4), the trough-like ventral process is distally expanded, subtruncate, when viewed from ventral aspect it is bifid for only a short distance.

Holotype.—Male; Oakland, California; April 17, 1915; (E. P. Van Duzee). This specimen, Type 4635, is in the collection of the California Academy of Sciences, San Francisco, California.

***Homoplectra spora* n. sp.**

This, the fifth species to be described in the genus, can easily be distinguished from *oaklandensis* (Ling) it's closest described relative on the basis of the armature of the aedeagus. In *spora* the lateral spur-like process is short, thick and directed ventrad, while in *oaklandensis* it is long, slender and directed dorsad.

Male. Length 10 mm. Head, thorax and base of legs blackish, remainder of legs yellowish, antennae and wings brown. A pair of flat, narrow filamentous processes extend caudad from the fifth sternite.

Genitalia as in fig. 4. Sternal portion of ninth segment more heavily sclerotized than tergal portion. Acute dorsal horn-like process of tenth tergite does not reach level of tergite. Claspers long, slender, distally widened; apices turned abruptly mesad, apex covered with brownish peg-like teeth. Aedeagus armature as follows, fig. 4a: a pair of dorsal arms rather heavily sclerotized, long, slender, acuminate, distally acute and flat from lateral aspect; near origin of dorsal arms a pair of laterally located thick heavily sclerotized hook-like spurs, apically acute and directed ventrad; aedeagus proper tubular, elongate, not heavily sclerotized,

Explanation of Plates

Fig. 1. Chimarra adella, male genitalia, lateral aspect; 1A, clasper, caudal aspect; 1B, tenth tergite, dorsal aspect; 1C, ventral flattened portion of aedeagus.

Fig. 2. Chimarra xesta, male genitalia, lateral aspect; 2A, clasper, caudo-ventral aspect; 2B, tenth tergite, dorsal aspect.

Fig. 3. Homoplectra oaklandensis, armature of aedeagus, lateral aspect.

Fig. 4. Homoplectra spora, male genitalia, lateral aspect; 4A, armature of aedeagus, lateral aspect; 4B, aedeagus proper, ventral aspect; 4C, ventral trough-like process, ventral aspect.

Fig. 5. Hydropsyche abella, male genitalia, lateral aspect, 5A, aedeagus, lateral aspect.

Fig. 6. Hydropsyche abella, female genitalia, lateral aspect.

Fig. 7. Cheumatopsyche gelita, male genitalia, lateral aspect; 7A, tenth tergite lobes, dorso-caudad aspect; 7B, tenth tergite lobes, caudal aspect.

Fig. 8. Cheumatopsyche gelita, female genitalia, lateral aspect; 8A, clasper receptacles, dorsal aspect.

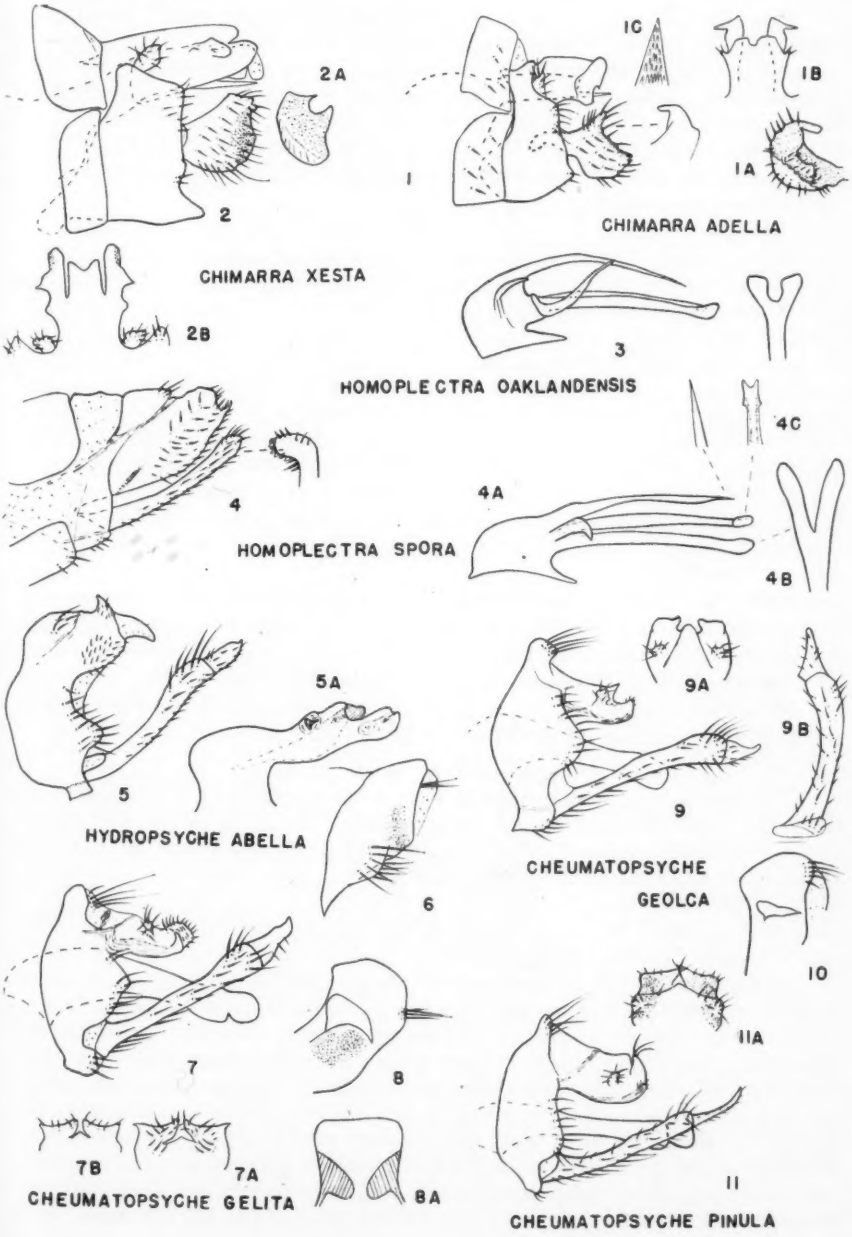
Fig. 9. Cheumatopsyche geolca, male genitalia, lateral aspect; 9A, tenth tergite lobes, dorso-caudal aspect; 9B, clasper, ventral aspect.

Fig. 10. Cheumatopsyche geolca, female genitalia, lateral aspect.

Fig. 11. Cheumatopsyche pinula, male genitalia, lateral aspect; 11A, tenth tergite lobes, dorso-caudal aspect.

apex curved slightly dorsal, from ventral aspect, fig. 4b, furcate apically; a ventral trough-like process in which the aedeagus fits, expanded distally and obtuse, from ventral aspect, fig. 4c, distal two-fifths bifid.

Holotype.—Male; Santa Cruz Co., Calif.; Mt. Herman; March 7, 1943; (Kenneth Frick).



Paratype.—Male; Portola State Park, San Mateo Co., Calif.; May 8, 1950; (E. S. Ross).

The holotype is deposited in the collection of the California Academy of Sciences, San Francisco, California.

***Hydropsyche abella* n. sp.**

This species belongs in the *amblis*, *tana* and *protis* group and is most closely related to *amblis* Ross. Outstanding differences is the shape of the apices of the tenth tergite, the position of the spur associated with the membranous dorso-lateral processes of the aedeagus, and the sinuate profile of the aedeagus when seen from the lateral aspect.

Male. Length 9-10 mm. Coloration of head, thorax and abdomen dark brown, antennae and legs light yellow, wings light brownish, irrorate with white marks mostly near apex.

Genitalia as in fig. 5. Ninth segment annular, lateral projection ovate, sternum narrowed. Tenth tergite with an acute dorso-mesal projection, reaching dorsad beyond remainder; apex divided into a pair of thin, wide processes, apically acute and directed ventrad, from dorsal or ventral view apices convergent. Clasper with apical segment short, its apex produced into an acute point. Aedeagus with base at right angles to remainder; dorsal plates ovoid, fig. 5a, heavily sclerotized, basad of these is a membranous area which terminates and becomes bulbous about midway length of structure, from bulbous areas arise a sessile caudo-laterad directed spur; apex produced considerably beyond dorsal plates, bearing one pair of extrusible spicules laterally, and a pair apically.

Female. Length 11 mm. Color and structure very similar to that of male. Genitalia as in fig. 6. Ninth segment with clasper groove relatively small, indistinctly concave; lateral lobe short, extending beyond margin of segment only a short distance.

Holotype.—Male; stream 2.5 miles up Warner Canyon from Highway 395, N. of Lakeview, Lake Co., Oregon: July 16, 1950; (Hugh B. Leech.).

Allotype.—Female; same data as for holotype.

Paratypes.—2 ♂, 1 ♀; same data as for holotype.

Holotype, allotype and one male paratype is deposited in the collection of the California Academy of Sciences, San Francisco, California.

***Cheumatopsyche gelita* n. sp.**

This species may be distinguished from other described Cheumatopsyche by the rather large obliquely situated lateral lobes of the tenth tergite, and by the short, wide apical segment of the clasper. The position of the tenth tergite lateral lobes bears some resemblance to those of *wrighti* Ross, but beyond that there is little similarity.

Male. Length, 8-9 mm. Head, thorax and abdomen brownish; antennae, legs and palpi fulvous. Wings uniformly light brown, only faintly irrorate.

Genitalia as in fig. 7. Ninth segmental tergum and sternum narrowed, lateral portion expanded. Tenth tergite longer than deep; from lateral aspect apico-ventral corner appears to terminate in an acute prong abruptly curved dorsad, lateral lobes rather large, lying obliquely against the tergite and directed cephalad, setae fairly abundant over surface, longest along apical margin; from dorso-caudal aspect, fig. 7a, apices sub-acute, not quite touching, dorso-lateral corner angulate, entire lobe short and wide; from caudal aspect, fig. 7b, dorsal margin concave, dorso-lateral corner acute. Lateral wart of tenth tergite appears to be double from lateral aspect, but when seen dorsally it is short, narrow and long. Claspers with basal segment elongate, expanded toward apex; apical segment short, only

gradually narrowed distally, apex sub-truncate; from dorsal or ventral aspect apices convergent. Aedeagus only slightly curved, base not bulbous.

Female. Length, 8.5-9.5 mm. Very similar to male in color and general structure. Genitalia as in fig. 8. Clasper receptacle large, opening to it occupying most of lateral aspect of ninth segment; when seen from dorsal aspect, fig. 8a, apex of clasper receptacle obtuse.

Holotype.—Male; Diamond Creek, White Mts., Arizona; June 21, 1950; (R. H. Beamer).

Allotype.—Female; same data as for holotype.

Paratype.—42 ♂, 3 ♀, same data as for holotype.

Holotype, allotype and twenty paratypes deposited in the entomology collection of the University of Kansas, Lawrence, Kansas.

***Cheumatopsyche geolca* n. sp.**

This species can easily be distinguished from others in the genus on the basis of the tenth tergite and its lateral lobes and the distinctive clasper. The apical segment of the clasper bears some resemblance to *enonis* Ross. The structure of the tenth tergite suggests a relationship to *analis* Banks.

Male. Length 5-7 mm., the majority about 6 mm; only a few of the described Cheumatopsyche are as small as this species. Color of head dark brown, thorax and antennae brownish, appendages straw yellow, wings fulvous.

Genitalia as in fig. 9. Ninth segment annular, tergum narrowed and produced into a pair of protuberances bearing a row of long setae along apical margin. Tenth tergum lightly sclerotized, indistinctly separated from ninth, longer than wide, near center of lateral aspect appears a rather wide, short wart bearing several long setae; apex truncate from lateral aspect, apico-lateral lobes do not extend above dorsal margin when seen from lateral aspect, seen from dorso-caudad view, fig. 9a, they are short, wide, widely separated, meso-apical apex subacute. Clasper with basal segment slender except slightly enlarged at the shoulder which bears the customary cluster of long setae; apical segment, seen from ventral aspect, fig. 9b, somewhat triangular, baso-mesal corner angulate. Aedeagus typical for genus; apical portion somewhat longer and apex more ovate than usual, basal bulb not enlarged.

Female. Length 7-8 mm. Very similar to male except usual antigenetic structures. Genitalia as in fig. 10. Clasper receptacle short and wide, apex directed dorso-cephalad.

Holotype.—Male; Humboldt River, near Carlin, Nevada; Elev. 4900; Aug. 21, 1950; (C. P. Alexander).

Allotype.—Female; same data as for holotype.

Paratypes.—Nevada: same data as for holotype, 2 ♀. Oregon: Snake River, near Huntington, June 5, 1948, D. G. Denning, 153 ♂, 14 ♀. Idaho: Snake River, near Hagerman, June 22, 1948, D. G. Denning, 4 ♂.

***Cheumatopsyche pinula* n. sp.**

This species may be distinguished from other members of the genus by its characteristic tenth tergite. In this species the ventral margin of the tenth tergite is broadly rounded, the apico-lateral lobes are curved cephalad and bears several prominent setae. In this respect it bears some resemblance to *lasia* Ross, but it differs from that species in many ways.

Male. Length 6.5 mm. Color of head and thorax light brown, legs and antennae fulvous, wings light yellow, faintly irrorate.

Genitalia as in fig. 11. Ninth segment annular, tergum and sternum considerably narrowed. Tenth tergite longer than deep, ventral margin broadly

arcuate; apico-laterad lobes curved cephalad and not quite reaching dorsal margin, bearing three rather long setae curved caudad; near base of lateral lobes there appears to be a short digitate-like process bearing short setae which is almost imperceptible from dorso-caudal aspect. When viewed from dorso-caudad aspect, fig. 11a, apico-lateral lobes are short and wide, dorsal margin slightly concave, apices confluent. Claspers long and slender, basal segment almost three times length of apical segment which is very slender, tapering gradually dorsad when viewed laterally. Aedeagus typical for genus.

Holotype.—Male; Carrizo Creek, near Carrizo, Arizona; June 6, 1950; (R. H. Beamer).

Holotype deposited in the entomology collection of the University of Kansas, Lawrence, Kansas.

A Review of Studies of Blood-Sucking Flies in Northern Canada[1]

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Introduction

Interest in the Canadian north has increased greatly during recent years because of its importance in national defence, and the actual and potential value of its natural resources. A serious hindrance to northern development is the presence during the short summer season of great numbers of blood-sucking flies that make life difficult for man and beast. The major pests in the forested sub-arctic are mosquitoes, black flies, and tabanids (the so-called deer flies and moose flies); beyond the tree-line on the arctic tundra black flies are much less troublesome and tabanids do not occur, but in many regions mosquitoes are a terrible scourge.

Comparatively little was known about these insects before 1947. In that year, at the request of the Defence Research Board, and with their financial assistance and co-operation and the co-operation of various other agencies, the Division of Entomology commenced a study of the biology and control of the northern species of biting flies.

A panel of experts known as the Entomological Research Panel, of the Defence Research Board, was appointed in that year to review and recommend programs of research, with Dr. W. E. van Steenburgh, Associate Director of Science Service, Department of Agriculture, as Chairman; and Mr. A. C. Jones, of the Research Co-ordination Staff, Defence Research Board, as Secretary.

The investigations are conveniently divided into four categories: (1) the northern insect survey, dealing mainly with the taxonomy and distribution of biting flies and other species of insects, and primarily the responsibility of Systematic Entomology, Division of Entomology; (2) studies of the biology and control of biting flies, primarily the responsibility of Household and Medical Entomology, Division of Entomology; (3) botanical studies, carried out by the Division of Botany and Plant Pathology, Department of Agriculture, and (4) special projects, undertaken by various universities supported by grants-in-aid from the Defence Research Board. This paper deals chiefly with the biology and control studies.

A summary account of the investigations carried out in northern Canada

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in 1947, 1948, and 1949 has been published (Twinn, 1950), and papers on various phases of the work during that period and since have appeared in scientific journals.

Biological Studies

Studies of the biology of northern biting flies have been carried out most intensively at Fort Churchill, Man., where species typical of both the forest and the tundra occur. Other biological studies have been pursued in forested regions, at Goose Bay, Labrador, and at Whitehorse, Y.T. Much information has been obtained on the life-histories, habits, species associations, relative abundance, and status as pests of the various species, and some of this has been published (see "References").

There are five genera of mosquitoes in the sub-arctic and one in the arctic. The species that are most troublesome to man and animals throughout the North belong to the genus *Aedes*. These insects have only one generation a year, and spend the long winters in the egg stage beneath the snow and ice in low places subject to flooding in the spring (Twinn, 1949).

Attempts to rear the northern species of *Aedes* from one generation to another under laboratory conditions for experimental purposes have so far met with failure, although progress in this direction has been made at the Defence Research Northern Laboratories, Fort Churchill, by Mr. W. E. Backel.

Black flies (Simuliidae) in northern Canada belong to several genera. The life-histories and habits of the various species differ considerably. All breed in running water, but some favour small streams, whereas others prefer the larger rivers; one species, *Simulium pictipes* Hgn., is found only in waterfalls. The winter is passed in the egg stage by certain species and in the larval form by others; some have only one generation each year, others have two or more. Information is still meagre on the flight ranges and host preferences of the various species. At least two, both widely distributed and abundant species, are serious pests of man, namely, *S. venustum* Say and *Prosimulium hirtipes* Fries.

Tabanids appear to attack animals more readily than they do man, but under favourable conditions they are very troublesome pests. Sixteen species belonging to three genera have been studied at Churchill (Miller, 1951). The females lay their eggs in masses on vegetation overhanging pools. The larvae of *Tabanus* spp. feed on small animal life such as insect larvae and snails; the larvae of *Chrysops* spp. appear to obtain their food from the organic matter present in the moss of their natural habitat. The winter is passed in the larval stage below the surface of vegetation. Most of the species have a life-cycle of at least three years.

The Food of Biting Flies

An interesting and important question is the source of food of female mosquitoes and other northern biting flies and whether a blood meal is essential to reproduction. It was deduced from the presence of pollinia attached to the eyes of female mosquitoes of most of the species at Churchill that a large majority of them visit the flowers of the northern orchid, *Habenaria* spp., for nectar (Twinn *et al.*, 1948; Hocking *et al.*, 1950). Mosquitoes have also been observed probing in the flowers of other species of plants, apparently for nectar. Dr. A. S. West, Queen's University, Kingston, Ont., in laboratory experiments at Churchill, showed that mosquitoes, *Aedes communis* (DeG.), that fed on flowers activated with radio-active phosphorus (P^{32}) took up sufficient material, probably nectar, to become highly radio-active.

It is also important to identify the warm-blooded hosts of the various species to determine their relative significance as pests of man and animals and their possible significance as disease vectors. An attempt is being made to do this by

means of the serological procedure known as the precipitin test (West, 1950) in a joint project of the Division of Entomology and Queen's University under the leadership of Dr. West. Samples of sera have been obtained from mammals and birds in the Churchill area, and anti-sera have been prepared from a number of them. Studies by Mr. G. S. Eligh on the application of the precipitin test for this purpose (Eligh, 1951) have led to improved techniques that should greatly facilitate identification of the blood meals of insects and other blood-sucking arthropods and should have useful application in other regions.

The use of the precipitin test for identifying plant hosts of biting flies and for determining the predators of biting flies is also being investigated.

Dispersal and Flight Range

Knowledge of the distances that species of mosquitoes and other biting flies are likely to disperse from their breeding areas is important in control projects. Some species of mosquitoes, especially the typical forest species, are known to be relatively sedentary in habit and to remain near their point of emerging; others, particularly salt marsh, prairie, and tundra species, may fly or be carried on the wind many miles from their breeding places (Twinn, 1949).

In 1950, radio-active phosphorus (P^{32}) was used as a means of tracing the dispersal of mosquitoes at Churchill, and of black flies at Saskatoon, Sask. The study at Churchill was carried out by Drs. D. W. Jenkins and C. C. Hassett, of the Army Chemical Center, Maryland, in co-operation with the Division of Entomology and the Defence Research Board. About 3,000,000 adults of *Aedes communis*, made radio-active by treating the larvae in wooden rearing tanks, emerged and dispersed in the swampy forest south of Churchill. Only 141 radio-active specimens were recovered in subsequent collections; on the basis of these recoveries it was concluded that the effective dispersal of this forest species is about one-quarter of a mile, indicating only a limited flight range (Jenkins and Hassett, 1951).

Because of the lack of facilities on the tundra, the Division of Entomology, in co-operation with the University of Saskatchewan, is continuing this study from Saskatoon in 1951 with migratory prairie species, to develop techniques and obtain data applicable to the tundra species. The dispersal and flight habits of black flies are also being studied by means of P^{32} at Saskatoon.

Correlation of Biting-fly Activity and Meteorological Conditions

The relation between meteorological conditions and the activities of biting flies is being investigated at Churchill, to obtain a better understanding of the factors that govern the activities of the insects, and to explore the possibility of forecasting their activity from data used in weather forecasting. The project is a co-operative one between the Division of Entomology and the Department of Zoology, University of Toronto. Mr. W. O. Haufe, Division of Entomology, Ottawa, who is in charge of the phase of the study relating to mosquitoes, reports that the 1950 data indicate that temperature is the most important factor limiting activity; the optimum range of temperatures for sub-arctic species of *Aedes* is 55°-60°F., the near optimum range extending five degrees above and below these temperatures; the lower threshold for activity is 36°-39°F., depending on the modifying effects of other meteorological factors such as wind, atmospheric pressure, and the moisture content of the air; in addition to modifying the effect of temperature, wind affects the efficiency of the mosquitoes in flight; winds at velocities of about 12 m.p.h. or greater completely prevent attacks on man; at 7 m.p.h. attacks are reduced by 50 per cent. Dr. F. P. Ide, Department of Zoology, University of Toronto, reported examples of black-fly activity diminish-

ing with lowered temperatures and ceasing completely at about 50°F.; wind speed appeared to be significant in regulating the times of flight of black flies; biting occurred only during the daylight hours; data were obtained on the influence of changes in atmospheric pressure on the insects' activities.

Biological Control

The natural enemies of biting flies are being studied at Churchill and at the Dominion Parasite Laboratory, Division of Entomology, Belleville, Ont., under the direction of Mr. H. G. James, to ascertain whether any of them can be manipulated to increase their effectiveness. They include parasitic and predacious insects, spiders, birds, fish, nematodes, bacteria, protozoa, and predacious plants (bladderworts). So far, the work has consisted of a preliminary survey. The serological precipitin test and the use of radio-active phosphorus are being investigated as methods of detecting and assessing the importance of predators of biting flies.

Chemical Control Studies

The results of experiments with various insecticides at Churchill and elsewhere in 1947 and 1948 showed that DDT in fuel oil was the most satisfactory material for use in mosquito control (Goldsmith *et al.*, 1949; McDuffie, Cross *et al.*, 1949; McDuffie, Sharp *et al*) and that as little as one-tenth of a pound of DDT per acre is effective against the larvae.

Area Control of Mosquitoes by Aircraft

In 1949, experiments in area control of mosquitoes were carried out in the vicinity of R.C.A.F. bases in forested areas of northern Canada. Areas averaging about nine square miles were sprayed with DDT in oil solution from a C47 aircraft at a dosage of approximately one-quarter of a pound of DDT per acre (Twinn *et al.*, 1950). To obtain satisfactory control about the bases, two applications were usually necessary, the first against the larvae in the breeding places and the second against the adult mosquitoes that survived the initial treatment or invaded the area from untreated areas. The average mortality of the larvae from these treatments was 91 per cent and that of adults 85 per cent.

A major difficulty in applying larvicides is that careful and accurate preliminary surveys of the breeding places are essential to success. Accordingly, in 1950, an experiment in area control by spraying only against the adult mosquitoes was carried out at Goose Bay, Labrador (Brown *et al.*, 1951). An area of approximately 19 sq. mi. surrounding the R.C.A.F. air base was sprayed with approximately one-sixth of a pound of DDT per acre at the end of June, when the mosquitoes were reaching a peak of abundance. There was an immediate reduction of about 90 per cent, and for a month after the treatment the mosquito population in the sprayed area did not exceed 25 per cent of that in the unsprayed areas.

During 1950 aerial spraying was used successfully against flood water mosquitoes in the Ottawa and Winnipeg districts and other species at R.C.A.F. bases in the North West Air Command region (Brown and Twinn, 1950; Sharp, prepared for publication). Work is going forward at the Defence Research Suffield Experiment Station, Ralston, Alta., on the development of improved insecticide dispersal equipment for use on aircraft.

Control of Black Flies with Larvicides

An efficient method of controlling black-fly larvae in streams without harming fish was developed at Churchill in 1947 (Hocking *et al.*, 1949) and subsequently (Hocking, 1950). This consists of applying DDT in fuel oil solution at a concentration of one part of DDT to ten million parts of water

maintained at the point of application for 15 min. This method has since been used successfully to control the cattle-infesting black fly *Simulium arcticum* Mall. in Saskatchewan by treating the Saskatchewan River (Arnason *et al.*, 1949; Twinn, 1950), which is the chief source of this species in that province (Fredeen *et al.*, 1951). In May, 1950, a treatment of the river at this dosage practically eliminated black-fly larvae for a distance of 117 miles. Analysis of water samples taken at various points downstream showed minute amounts of DDT in solution (Berck, unpublished report). Air-dried silt from these water samples also contained DDT, suggesting that the insecticide may be carried long distances on particles of silt that resist sedimentation. Dissections of black-fly larvae indicated that they ingest relatively large quantities of silt; this may explain why the treatment is effective for such long distances in this river, whereas similar treatments in clear streams were effective for much shorter distances.

The control of one species of black fly by means of DDT larvicide having been demonstrated in a prairie region where there is only one main source of that species, it was desired to ascertain whether the method could be successfully employed in rough, hilly, forested country, where many species of black flies breed in numerous rivers and streams. Accordingly an experiment in area control was carried out in the region of Goose Bay, Labrador, where the bogs and forests are infested with enormous numbers of black flies belonging to about 25 species. Seventy-seven streams and rivers infested with black-fly larvae and flowing into or through a 200-sq. mi. area lying within an 8-mi. radius of the Goose Bay airport control tower were treated in June, 1950, with DDT in 10 per cent solution in fuel oil at the appropriate dosage. The treatments were applied from a U.S.A.F. helicopter, from points on the ground, and from a boat; the Hamilton River was treated from an R.C.A.F. Dakota C47. Biological assessments of the adult black-fly population made periodically from early July to the third week of August indicated that the treatments caused a large reduction in the black-fly infestation at the centre of the treated area and a significant reduction within a radius of four to six miles; however, even at the centre, black flies were sometimes troublesome late in the season (Hocking and Richards, prepared for publication).

Aerosols in the Control of Biting Flies

Investigations in 1949 and 1950 with aerosols emitted from thermal generators at Whitehorse, Y.T., Camp Borden, Ont., and Churchill, Man., showed that an aerosol containing 5 per cent DDT and with a mass median diameter particle size of 10 to 20 microns, applied at a rate of 20 gal. per hour from a vehicle travelling at 1 to 2 m.p.h., is effective against mosquitoes and black flies when applied under suitable meteorological conditions. For best results, the treatment of open areas requires atmospheric temperature inversion and a wind velocity of 2 to 3 m.p.h. Under these conditions the application has been effective to a depth of 500 yd. from the emission point, although 100 yd. is more usual. The treatment of wooded areas requires a wind velocity of 4 to 7 m.p.h. to obtain effective penetration of the forest vegetation (Peterson, prepared for publication). This investigation is being continued with several types of commercial thermal generators, gasoline engine exhaust generators, and mist sprayers.

Biting-fly Repellents

Field tests of promising biting-fly repellents applied to the skin were made at Churchill in 1947 and 1948 in collaboration with United States workers (Twinn, 1950). None was sufficiently superior to replace the compounds developed during the war years and now in general use and considered as standard repellents,

namely, dimethyl phthalate (DMP); 2-ethyl hexanediol (612), and a mixture of six parts of DMP, two parts of 612 (or two parts of dimethyl carbate), and two parts of Indalone, known as the 6.2.2 mixture. When impregnated into clothing none of the materials tested was effective for more than a few days.

In 1950-51 more than 200 candidate repellent compounds were synthesized in the Department of Chemistry, University of Toronto, by Dr. G. F. Wright and associates, under a research grant from Science Service, Department of Agriculture, Ottawa. Most of these have been assessed at Ottawa by standard biological methods with *Aedes aegypti* (L.) and human subjects. About a dozen were comparable to, or better than, dimethyl phthalate; these are being submitted to field tests in 1951 against native species of *Aedes*.

The possibility of orally administered substances acting as repellents and protecting animals and man from insect bites has been investigated at the Ontario Veterinary College by Dr. A. A. Kingscote under a Defence Research Board grant. A large number of compounds have been tested in this manner on laboratory animals, with *A. aegypti* as the test insect. So far, no substance has been found that will act in this way.

Responses of the Female Mosquito

It is believed that before further significant progress can be made in the development and use of repellents a more adequate understanding of their mode of action is required. Toward this end, laboratory studies of what attracts female mosquitoes to their hosts and what repels them have been made with *A. aegypti* by Dr. A. W. A. Brown and co-workers, at the University of Western Ontario, assisted by grants from the Defence Research Board. The results of these studies have been submitted for publication (Brown *et al.*, in press; Peterson and Brown, in press; Sarkaria and Brown, in press), and certain of them have been confirmed in the field with northern species of *Aedes* by Dr. Brown, by means of ingeniously contrived "dummy men" (Brown, 1951; Brown, in press). The factors that attract mosquitoes to bodies were found to include warmth (body temperature), moisture (when the air temperature is above 60°F.), carbon dioxide; perspiration (at a certain vapour concentration); and dark-coloured clothing. It was found that mosquito repellents are effective because of the repellency of their vapours, and that these vapours eventually kill mosquitoes confined in them; also that their repellent intensity is a function of their volatility and biological activity, and the duration of effect an inverse function of their vapour pressure (Sarkaria and Brown, in press).

Other Investigations

Other subjects under investigation include the psychological effects of biting-fly attacks and methods of personnel selection and training to overcome them; the nature of insect toxins and the possibility of immunizing the host against their effects; the development of protective clothing with special attention to material, colour, and design.

Summary

The biting-fly problem in the North, and the origin, purpose, methods, and results of research carried out from 1947 to 1950 by the Division of Entomology and co-operating agencies on behalf of the Defence Research Board are discussed. The major pests of the sub-arctic are mosquitoes, black flies, and tabanids; of the arctic, mosquitoes. The program has included studies on the species, their relative abundance, breeding places, life-histories, hosts, flight ranges, and natural enemies, and on the effects of meteorological conditions on their abundance and activity. Methods of chemical control on small and large areas have been developed, and tests have been made of protective clothing and repellents.

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Joint Meeting of the Entomological Society of Canada and the Entomological Society of Ontario, Ottawa, Ontario, November 1, 2, 3, 1951

The first annual meeting of the Entomological Society of Canada and the 88th annual meeting of the Entomological Society of Ontario were held jointly in the Chateau Laurier and Carleton College on the first, second and third of November, under the presidency of Mr. W. A. Ross for the Entomological Society of Canada and Mr. W. N. Keenan for the Entomological Society of Ontario.

The meetings were opened in the Chateau Laurier on the morning of November 1, with an address of welcome by Dr. J. G. Taggart, Deputy Minister, Canada Department of Agriculture. The remainder of the session and the two following sessions were devoted to invitation papers covering a wide range of subjects. In the evening of November 1, a round-table discussion on the IXth International Congress of Entomology, between members who had attended the Congress, took place in the Science Service Building. Sectional meetings, covering agricultural entomology, under the chairmanship of Dr. Robert Glen, forest entomology, under the chairmanship of Mr. J. J. de Gryse, and general entomology, under the chairmanship of Professor A. W. Baker, were held in Carleton College, on November 2. Some thirty papers were presented or read by title at these meetings. The proceedings ended with business meetings of the two societies on the morning of November 3.

On Thursday, November 1, a tour of the Parliament Buildings was arranged for the lady visitors and on Friday they were taken on a tour of Ottawa and Hull, followed by a tea at the Champlain restaurant. The banquet of the societies was held on Friday evening in the Chateau Laurier. It was attended by some 280 members and guests, including Dr. Taggart, Deputy Minister of Agriculture, Mr. H. L. Trueman, Director of Administrative Services, Dr. W. E. van Steenburgh, Associate Director of Science Service, Dr. M. M. MacOdrum, President of Carleton College, Professor Charles Palm of Cornell University and Dr. H. H. Ross of the University of Illinois. The banquet was followed by an entertaining programme of talks and music, of which the highlights were Professor Baker's reminiscences, the discourse on limericks by Mr. Atkinson, Principal of the Glebe Collegiate, the French-Canadian songs by the students of the Collège St. Alexandre, assisted by some of our members, and the piano-accordion solos by Mr. G. P. Holland as an accompaniment to the community singing. Mr. Dick Painter also made a notable contribution to the success of the evening as an able Master of Ceremonies.

The members of the programme committee, of which the undersigned was chairman, are inclined to regret that their efforts to provide a solid diet of entomological information were so successful that little time was left for the social intercourse that is, after all, the most important feature of scientific gatherings. However, they felt that the first meeting of the Entomological Society of Canada should keep proceedings on a high scientific level. Next time the members may be let off more lightly.

We are sure that all the members of the two societies, whether they were able to attend the meetings or not, will be glad to have the delightful photographic record reproduced in the insert to this number. It shows what a candid camera can do in the hands of an artist-psychologist like Mr. C. H. Kenney.

W. R. THOMPSON.

A New Braconid from Mexico[1]

By JOHN C. MARTIN[2]
Dominion Parasite Laboratory
Belleville, Ontario

In revising the genus *Triaspis* Haliday, as found in North America north of Mexico, the author studied an undescribed species from Mexico. This form is a parasite of *Apion godmani* Wagner, a weevil of economic importance. This paper provides a name and a description for this new parasite.

The author is most grateful to Dr. V. S. L. Pate of Cornell University for his helpful criticism of the manuscript, and also to Mr. C. F. W. Muesebeck of the U.S. National Museum for the loan of material, and to Dr. Arthur C. Smith of Cornell University for the specimens collected while he was studying the biology of the host in Mexico.

***Triaspis azteca*[3] sp. n.**

This Mexican form has the general habitus of its closest relative, *Triaspis pissodis* Viereck, but may be readily distinguished by the darker colour of the legs, by the completely impunctate area of convergence of the notauli, and by the obtuse median tooth of the anterior border of the clypeus.

Holotype, ♀.—Length 2 mm. Black, the following stramineous: mandibles except the tips, which are blackish; palpi; fore and middle legs except for darkish stains on femora and the tarsal segments; hind trochanters; and basal third of hind tibiae.

Head sparsely covered with fine silvery hair, completely nitidous; fronto-clypeal groove distinct, clypeus with large punctures, anterior edge with a raised border bearing an obtuse median tooth. Antenna 18-segmented; pedicel half as long as the scape; first flagellar segment four-fifths as long as the second; antennal segments 3 to 9 and apical segment longer than broad, the remainder broader than long.

Thorax nitidous, black, and smooth; notauli strongly impressed with fine foveae in the troughs, converging but reaching neither each other nor the scutellar groove, and the area of convergence completely impunctate; scutellar groove broad, shallow, and smooth, and with a median longitudinal carina; scutellum impunctate, shining, and flat with sloping sides; propodeum roughly rugose, with a sharply declivous profile, and clothed with long silvery hair; dorsal surface of propodeum with a short median longitudinal carina which divides and delimits on each side a rather narrow transverse impunctate area; the posterior surface presenting a median pentagonal area bounded by carinae; lateral protuberances broad, compressed, and not strikingly evident; mesopleura impunctate, smooth, and shining; upper portion of the episternum clothed with a short silvery pile; mesopleural groove broad, shallow, impunctate, and sinuate; upper parts of propleura smooth, lower portions irregularly punctured. Tegulae dark brownish. Wings hyaline with the veins of the proximal third of each fore wing nearly colourless except the costa and stigma, which are brownish; second abscissa of radius pale, in contrast with the first abscissa and the first transverse cubital vein, which are darker in colour. Legs long; all femora rather broad; all tarsi slender, and all tarsal claws with a broad basal tooth.

Abdomen rather short and broad; first and second tergites longitudinally rugose, as is the third laterally, the remainder of the third being smooth and

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[3]In allusion to the ancient Aztecs, who dwelt in the country before the advent of the Spaniards.

shining; all three tergites with a lateral ridge, which in the third tergite is expanded into a broad, transparent, marginal lamella surrounding the apex of the abdominal carapace; no movable joint between the first and second tergites. Ovipositor and sheath slender and extending beyond the tip of the abdomen by half the length of the latter.

Allotype, ♂.—Similar to the holotype except for the customary sexual differences, and with the propodeum more rugose and the antenna 20-segmented.

Holotype.—Chapingo, Edo. de Mexico, Mexico, 2160 metres, December 5, 1946. (J. J. McKelvey; reared from *Apion godmani* Wagner). [U.S.N.M. Type Catalogue No. 61142]

Allotype.—Topotypical, with other data the same as for the holotype. [U.S.N.M.]

Paratypes.—Mexico: 11 ♂ ♂, 7 ♀ ♀, topotypical, with other data the same as for the holotype [U.S.N.M.]; 1 ♂, 5 ♀ ♀, topotypical, 2160 metres, 12-IX-46; 11 ♂ ♂, 7 ♀ ♀, topotypical, 2160 metres, 11-IX-46; 1 ♀, Kilometre 70 from Mexico City to Cuautla, Morelos, 2270 metres, 4-XI-48; 3 ♂ ♂, Kilometre 65 from Iguala to Teloloapan, Guerrero, 1400 metres, 5-XI-48; 5 ♂ ♂, 7 ♀ ♀, Kilometre 325 from Mexico City to Acapulco, Guerrero, 1200 metres, 6-XI-48; 1 ♂, 1 ♀, Kilometre 19 from Acapulco to Zihuatanejo, Guerrero, 5 metres, 6-XI-48; 1 ♀, on highway from Iguala to Teloloapan, Guerrero, 1600 metres, 13-X-49; 2 ♂ ♂, Kilometre 386 from Mexico to Oaxaca, 900 metres, 8-XI-49; 3 ♂ ♂, Valle de Bravo, Edo. de Mexico, 1500 metres, 10-XI-49; 1 ♂, 5 ♀ ♀, Kilometre 68 from Mexico City to Cuernavaca, Morelos, 17-XI-49. [C.N.C.]

A total of 77 specimens (43 ♂ ♂, 34 ♀ ♀) of paratypic material were examined. Those in the U.S. National Museum are pinned specimens and agree with the types structurally and in general colour. The paratypes in the Canadian National Collection are preserved in alcohol and they agree structurally with the types although some variation was noted, particularly in the number of the antennal segments, 19-20 in the males and 18 19 in the females, and also in the amount of dark brown in the legs and in the extent of the rugosity of the third tergite.

Zoological Nomenclature: Applications relating to entomological nomenclature recently submitted to the International Commission on Zoological Nomenclature.

The following are insect names involved in recent applications to the International Commission on Zoological Nomenclature. The applications concerned have been published in detail during the current year, in several Parts of Volume 2 of the *Bulletin of Zoological Nomenclature*, as noted below. The majority of the applications, to become effective, involve the possible use by the International Commission on Zoological Nomenclature of its plenary powers, and hence may be of particular interest to specialists in the groups concerned. Any persons desiring to comment on applications are invited to do so in writing to the Secretary to the International Commission (address: 28 Park Village East, Regents' Park, London, N.W.1 England) as soon as possible. The latest date at which comments on applications should reach the Secretariat of the International Commission is given for each group of applications as follows: applications in Parts 1, 2, 4, October 20, 1951; applications in Part 5, November 4, 1951; applications in Parts 9/10, February 15, 1952. Comments received in sufficient time will be published in the *Bulletin of Zoological Nomenclature*. Comments received too late for publication but before the final date designated will be brought to the

attention of the International Commission at the time of the commencement of voting on the application in question. Every comment should be clearly marked with the Commissions' File Number as given below.

Applications appearing in Bulletin of Zoological Nomenclature, Vol. 2

Part 1 (Published Apr. 20, 1951).

Aphidius Nees, 1818 (Class Insecta. Order Hymenoptera) (pp. 18-20) (File 149).

On the question of the availability of the generic name *Leptopsylla* Rothschild & Jordan, 1911 (Class Insecta, Order Siphonaptera) (pp. 21-22) (comments, pp. 22-25) (File 166).

Proposal for suppression of the trivial name *ajax* Linnaeus, 1758 (as published in the combination *Papilio ajax*) (Class Insecta, Order Lepidoptera) (pp. 26-30) (File 192).

Part 2 (Published Apr. 20, 1951).

Rantus Dejean, 1833 (Class Insecta, Order Coleoptera) (pp. 40-45) (File 171).

Acantholyda Costa, 1894 (Class Insecta, Order Hymenoptera) and *Acanthocnema* Becker (Class Insecta, Order Diptera) (p. 46) (File 175).

Rhina Latreille, [1802-1803], and *Magdalis* Germar, 1817 (Class Insecta, Order Coleoptera) (pp. 47-55) (File 202).

Proposed addition to the *Official List of Generic Names in Zoology* of the names of thirteen genera in the Order Collembola (Class Insecta) pp. 56-58) (File 207).

Part 4 (Published Apr. 20, 1951).

Capsus Fabricius, 1803 (Class Insecta, Order Hemiptera) (pp. 103-104) (File 211).

Tettigonia and *Acrida*: proposed validation as from Linnaeus, 1758 (Class Insecta, Order Orthoptera) (pp. 106-109) (comments, pp. 109-118) (File 328).

Part 5 (Published May 4, 1951).

Tendipes Meigen, 1800, and *Chironomus* Meigen, 1803 (Class Insecta, Order Diptera) (pp. 134-138) (comments, pp. 138-139) (File 197).

Dorilas Meigen, 1800, and *Pipunculus* Latreille, [1802-1803] (Class Insecta, Order Diptera) (p. 140) (comments, pp. 141-149) (File 221).

Tendipes Meigen, 1800, and *Chironomus* Meigen, 1803 (Class Insecta, Order Diptera) (pp. 150-151) (comments, p. 152) (File 469).

Phlia Meigen, 1800, and *Dilophus* Meigen, 1803 (Class Insecta, Order Diptera) (pp. 153-154) (comments, p. 155) (File 498).

Tylos Meigen, 1800, and *Micropeza* Meigen, 1803 (Class Insecta, Order Diptera) (pp. 156-159) (comments, p. 160) (File 501).

Parts 9/10 (Published August 15, 1951).

Eysarcoris Hahn, 1834 (Class Insecta, Order Hemiptera) proposed validation of existing nomenclatorial practice (pp. 294-295) (File 212).

acuminata Ioff & Tiflov, 1946, as published in the combination *Rhadinopsylla* (*Rectofrontia*) *acuminata* (Class Insecta, Order Siphonaptera) (pp. 296-297) (File 386).

At the request of the Editor, the foregoing summary has been prepared from several recent submissions to *The Canadian Entomologist* by Francis Hemming, Secretary to the International Commission on Zoological Nomenclature.

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